A proposal to quantify and map the distribution, abundance, and population concentrations of sensitive grassland birds in two CALFED Bay−Delta Ecological Management Zones

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0099: A proposal to quantify and map the distribution, abundance, and population concentrations of sensitive grassland birds in two CALFED Bay−Delta Ecological Management Zones

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The objectives of the project are to quantify the distributions, relative abundances, and habitat associations of grassland birds across the Yolo Basin Ecological Management Zone and the North Delta Ecological Management Unit for the Delta Ecological Management Zone, and to evaluate the feasibility of expanding the bird monitoring protocol to the entire Bay−Delta region. Although rather local in scope, the proposed research appears timely given the lack of information about grassland birds in California and particularly the Bay−Delta region. The goals and objectives are clearly stated and consistent with the proposed methods, but it is not clear that the hypotheses presented are truly being tested with the proposed methods. Overall, the conceptual model is not well developed, the hypotheses are weakly linked with the goals and objectives, and the connection of hypotheses to research methods is also weak. Reviewers were concerned about the lack of detail described in the approach of the project. The methods are not thoroughly described nor justified, and it is not clear if the sample size is sufficient for addressing the hypotheses. For example, the authors propose using four different methods for surveying birds, but it is unclear why each is used. Most of the information could be obtained from

two methods: point counts and road surveys (for raptors). Furthermore, to estimate habitat associations proper sampling design is critical, but there is little description for their sampling approach. The analysis and interpretation is also not well documented. There is no mention of what local and landscape factors will be addressed, what agricultural habitats will be considered, how "hotspots" will be estimated (or defined), or how population concentrations will be estimated. This project is generally feasible and straightforward, the budget is generally justified but somewhat high for a survey project, and the investigators (Audubon) have capabilities in field data collection. However, the investigators have not demonstrated extensive experience in organizing and managing a project of this size, or in producing valuable products, such as peer−reviewed publications. The information obtained will be generally useful in filling a gap about habitat use by grassland birds of the region. The authors do not describe how this information will be useful in guiding policy decisions and how the information will be integrated effectively into the local policy formulation process.

Additional Comments:

The objectives of the project are to quantify the distributions, relative abundances, and habitat associations of grassland birds across the Yolo Basin Ecological Management Zone and the North Delta Ecological Management Unit for the Delta Ecological Management Zone, and to evaluate the feasibility of expanding the bird monitoring protocol to the entire Bay−Delta region. Although rather local in scope, the proposed research appears timely given the lack of information about grassland birds in California and particularly the Bay−Delta region. The goals and objectives are clearly stated and consistent with the proposed methods, but it is not clear that the hypotheses presented are truly being tested with the proposed methods. Overall, the conceptual model is not well developed, the hypotheses are weakly linked with the goals and objectives, and the connection of hypotheses to research methods is also weak. Reviewers were concerned about the lack

of detail described in the approach of the project. The methods are not thoroughly described nor justified, and it is not clear if the sample size is sufficient for addressing the hypotheses. For example, the authors propose using four different methods for surveying birds, but it is unclear why each is used. Most of the information could be obtained from two methods: point counts and road surveys (for raptors). Furthermore, to estimate habitat associations proper sampling design is critical, but there is little description for their sampling approach. The analysis and interpretation is also not well documented. There is no mention of what local and landscape factors will be addressed, what agricultural habitats will be considered, how "hotspots" will be estimated (or defined), or how population concentrations will be estimated. This project is generally feasible and straightforward, the budget is generally justified but somewhat high for a survey project, and the investigators (Audubon) have capabilities in field data collection. However, the investigators have not demonstrated extensive experience in organizing and managing a project of this size, or in producing valuable products, such as peer−reviewed publications. The information obtained will be generally useful in filling a gap about habitat use by grassland birds of the region. The authors do not describe how this information will be useful in guiding policy decisions and how the information will be integrated effectively into the local policy formulation process.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Hypotheses were very general. Hotspots were expected, but it was not clear what a hotspot was. Not enough detail or justification for hypotheses or ideas presented.

No definition of habitat associations. Not clear what congregatory species are or why the species would be congegatory. Another problem identified was that the reviewers did not expect peer−received scientific publications to result from this work.

Rating: inadequate

Technical Review #1

proposal title: A proposal to quantify and map the distribution, abundance, and population concentrations of sensitive grassland birds in two CALFED Bay−Delta Ecological Management Zones

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Goals, objectives, and hypotheses are clearly stated and internally consistent. Based upon the introductory information in the proposal, a need for information about grassland birds and their habitat associations has been identified, making the idea timely and important.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**A paucity of information about grassland birds of the region is sufficient justification for the study. The conceptual model, especially its assumptions, is described clearly. This appears to be a pilot project that will form a basis for extending the methodology to a larger study area within the CALFED region of interest. However, whether or not the project is a pilot or demonstration project per se, or a full−scale, but short−term implementation is not clear.**
Rating	

	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The rationale for selecting target species for monitoring relies heavily upon a set of metrics that have been derived from expert opinion approaches, which have limited repeatability due to the ambiguity of the methods involved, or a lack of explicitly stated quality control/quality assurance procedures, or both. The metrics in question involve Partners in Flight (PIF) "scores", Audubon WatchList status values, and estimates of "global" and state population sizes which are without confidence intervals or explicit discussion of the methods involved in deriving those estimates. In particular, the PIF approach to ranking has been professionally peer−reviewed and found deficient in numerous respects (see Auk 117(2): 549−561). The authors of this proposal make no attempt to qualify their basis for species choices in light of the inadequacies of the various ranking schemes upon which they rely for determining their target species for monitoring. In particular, the published peer review of the PIF ranking scheme is not acknowledged.** **There have been significant refinements to avian point count protocols as originally described by C.J. Ralph and others. In particular, the double−observer method proposed by Nichols et al. (Auk 117(2): 393−408), should be applied in this study. Using Nichols's approach will allow stronger inferences to be made regarding differences among and between habitats with respect to the responses of birds, as well as providing some information about observer error.**

	Additionally, there have been substantial advances made in estimating species likelihood of occurrence in samples (e.g. point counts) when species are imperfectly detected (i.e. the species is present, but not detected, as opposed to actually not being present in the sample). See MacKenzie et al. Journal of Animal Ecology 73(3): 546–555 (2004) for an introduction. Application of these methods and concepts to the proposed project would strengthen the inferences derived from the work. **A more detailed articulation of the habitat classification process would be helpful. Will a standard, nationally accepted and peer-reviewed "taxonomy" of land-cover (habitat) types be applied. An example of such a system is the National Vegetation Classification Standard, which is described at (http://biology.usgs.gov/npsveg/nvcs.html).**
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The description of the approach is lacking in detail and does not allow for an assessment of the extent of work involved during any given season. It is not clear how many sites will be chosen for monitoring, making an estimate of the effort required difficult. The rationale for choosing whether to apply point counts, line transects, or area searches to a given area is not clear. However, the rationale for using roadside surveys for raptors is straightforward and clear. There is no procedure outlined for accessing private property, other than existing Audubon California cooerators, for survey work. The area of property and its spatial dispersion, as represented by Audubon's Landowner Stewardship Program cooperators in the area, is not stated.**

Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**See comments under Feasiblity, above. Details of plans for interpreting the monitoring data are not clear.**
Rating	**fair**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The basic, descriptive information obtained will be useful in filling an information void about habitat use by grassland birds of the region. Whether or not the information will be useful in guiding policy decisions and how the information will be integrated effectively into the local policy formulation process is not clear.** **Plans for dissemination of the information are described, but no plan for assessing the effectiveness of the various dissemination methods is outlined. Without an evaluation process for the outreach components of the proposal, it will be difficult to determine how effective they will be.**
Rating	**fair**

Additional Comments

Comments	**Some of the methodological questions could be answered with a one year pilot/feasibility project, which would**

	allow for a better assessment of the real effort involved to meet the data collection goals of the project.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**It does not appear that there is any substantial track record or experience among the investigators in implementing and coordinating an effort of the scope and complexity described in this proposal. Daniel Cooper's record of publications relevant to the substance of this proposal is somewhat modest, as is that of the other personnel.**
Rating	**fair**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**In general, the budget seems reasonable and adequate. If the position of Director of Bird Conservation for Audubon California, occupied by Daniel Cooper, is a salaried position, it is not clear to me why salary recovery, in the amount of $12,990, plus fringes, over three years,is requested. It seems that an amount of that size could be provided by Audubon California as a "good faith" contribution to the project.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**I offer an overall summary rating of "fair" for this proposal. The information need which the proposal addresses is clear and important. However, a number of weaknesses of the proposal lead me to question whether the quality and applicability of the information gathered would be adequate to meet that information need. I recommend that a one year pilot study be funded, at a considerably reduced dollar amount, so that methods and feasibility can be tested and a better idea of the scope and costs of the tasks enumerated can be obtained.**
Rating	**fair**

Technical Review #2

proposal title: A proposal to quantify and map the distribution, abundance, and population concentrations of sensitive grassland birds in two CALFED Bay−Delta Ecological Management Zones

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The proposed research appears timely given the lack of information about grassland birds in California and particularly the Bay−Delta region. The goals and objectives are clearly stated and consistent with the proposed methods, but it is not clear that the hypotheses presented are truly being tested with the proposed methods. Hence, some internal inconsistency appears to be present. For example, in H1, grassland bird distributions are proposed to be related to a suite of landscape factors. However, there is no protocol outlined for the collection of data (or use of existing data) on these landscape factors. Please see "approach" section for more detail. In H2, the proposal suggests that certain types of grassland habitats are more productive than others for grassland birds, yet it is not clear that the data will be collected in a manner that would allow the evaluation of this hypothesis. Again, please see the sections that follow. H3 proposes the existence of grassland bird "hotspots," yet nothing in the proposal outlines how such hotspots will be distinguished from less diverse areas—in other words, no cutoff or threshold is identified, nor is a method proposed for determining a cutoff. Overall, the hypotheses have a shaky match with the goals and objectives, and the connection of hypotheses to research methods is similarly weak.**

	Overall, the project has simple but worthy goals—to yield information on the distribution and habitat relationships of grassland birds in the study area. This is an important first step, but a more ambitious project would have included measurements of reproduction and survivorship. Studies have shown that reliance on abundance to indicate the quality of habitat and the potential for successful reproduction typically falls short in areas with high human disturbance, where animals may fall into an ecological trap. Management decisions based on abundance alone run the risk of incorporating incorrect assessments of habitat suitability. I would have liked to see explicit recognition of this in the proposal.
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is justified given the lack of information about grassland birds in the Bay−Delta region. The study could provide, with a few clarifications and adjustments, the preliminary information needed to understand distributions and abundances of grassland birds in the region.** **The conceptual model is limited in its usefulness. It consists solely of six hypothesis−like statements, not all of which relate to what is being tested. A figure would have been useful here, showing the specific factors (specific elements of vegetation structure, other species as predators or competitors, landscape−level factors) expected to affect grassland birds (or, lacking this information, birds in general), the direction of those effects (positive or negative), and the parameters of avian populations expected to be affected (diversity, abundance,**

	reproduction). Some of the statements more clearly underlie reasons for the decline of grassland birds ("some grassland birds have minimum area requirements…"; "grassland species require a matrix of habitat types…"; etc.) than others. In a general sense, the conceptual model should be a large, interconnected series of thoughts about the way the world works, and should be used to generate specific testable hypotheses. When specific hypotheses are supported or rejected, the conceptual model can be updated. The link between the existing conceptual model and the proposed hypotheses is somewhat weak. The proposed research will allow little updating of the conceptual model, and suggests that the hypotheses have not been thoroughly worked out and do not derive from the conceptual model. **This project probably doesn't need a pilot, as the methods are standard and the sampling design is relatively simple. Therefore, full-scale implementation seems appropriate.**
Rating	**fair**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach generally matches the objectives well. The objectives of the project are to assess the distributions and relative abundance of grassland birds across the study area, and the methods proposed should yield that information. However, there are many details that are unclear, especially if the proposed research hypotheses are to be fully tested. As mentioned above, hypothesis H1 mentions several landscape-level variables, but it is unclear where these data will come from. The proposed vegetation**

sampling methods yield local-scale vegetation parameters only. Will GIS data be used? The budget alludes to this. If so, are those data available already, or will they need to be collected? What variables will be calculated, and how will they be used to model species distributions, diversity, or abundance? Hypothesis H2 indicates that the researchers wish to compare among habitat types, yet nothing in the sampling design accounts for this. It is unclear whether sample sites will be stratified by grassland type, which would likely be necessary to ensure sufficient sample sizes in each type. While 40-60 sites per year for two years may yield sufficient sample sizes for each of the seven habitat types listed in the project area description, this can only be accomplished through a deliberate stratification by habitat type during sample site selection (or through post-stratification, but this can be dangerous in that sample sizes might not turn out to be sufficient by type). Further, it is unclear precisely which habitat types are of interest, as the different types of grassland are referred to only in the project area description. I would suggest that in a natural history survey such as this, several habitat types expected to be important should be explicitly identified in advance, and sampling should be designed to meet information needs for each habitat type.

In addition, the decision to use random placement of point counts is not explained, as opposed to using a systematic design with a random start, which has some benefits in terms of ease of travel between points. It is unclear why transects, point counts, and area searches were all deemed necessary, although one could imagine that a perceived difference in efficiency drove the selection of transects for extensive monitoring and point counts for habitat relationships. Transects are definitely the preferred method for using distance sampling (if that in fact is what's proposed; it's not stated) in grasslands. Perhaps it would be preferable to tie a habitat protocol to the

transects rather than conduct additional point count surveys and area searches and do the associated releve? This decision depends a lot on the length of transects and effort expended (also not specified). It is unclear what the area searches are meant to add. And how will the results of these three methods be combined or compared, or will they be treated separately? Not enough description of and justification of the methods is included to know whether the three survey methods are necessary.

Roadside (driving) surveys for raptors seem an appropriate choice, but as with all road−based surveys, detections may not be representative of all grassland habitat, only areas with roads within them. Thus, the extrapolation of these surveys to habitat not surveyed must be done with extreme caution. I would have liked to see some treatment of this issue—as with most of the methods, these important details are glossed over or ignored. Additionally, in many areas, raptors are not best surveyed in the morning as proposed, but perhaps in the heat of summer in the Central Valley this is an appropriate time—something to consider.

Finally, the directed surveys proposed for sensitive species that congregate are not described at all. What is the protocol for the identification of survey sites, and what are the census methods? What species are targeted?

With some improvements to or better justification for the study design, the results could improve our knowledge of habitat relationships and distributions of sensitive grassland bird species in the Bay−Delta region. Basic information such as this will ultimately be very useful to decision makers. The identification of locations where sensitive species are concentrated also could be vital if these areas turn out to be threatened by development or habitat disturbance. The project has an encouraging potential to lead to a

	region−wide grassland bird monitoring program, especially one that incorporates nest monitoring and possibly mist−netting.
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	See above for information about the approach that is lacking, making the approach not fully documented. I believe the project to be technically feasible—the methods are standardized and relatively simple. The project has a high likelihood of success, but only if the approach is worked out a little better beforehand, including how best to engineer the sample to enable comparisons of grassland types. A more detailed treatment of the methods is necessary to assess whether the project can test the proposed hypotheses. The scale of the project is indeed consistent with the objectives.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	N/A
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The project is likely to produce products of value, as explained above. Basic information on distributions of grassland birds and their habitat associations will be a valuable product in terms of natural history as well as conservation. Data will be incorporated into the CALPIF database, part of the NBII, which will make the data available to a wide range of conservation practitioners.**
Rating	**good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The research team represents a good combination of experience in California grasslands and with California birds in general. The authors seem qualified to implement the project, and Audubon California should have the infrastructure necessary to support the proposed research.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	

	The budget seems completely justified and sufficient for the proposed work.
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I have given the proposal a “fair” rating overall to reflect my criticisms of the lack of detail in the study design, preventing a better understanding of the ability of the proposed research to meet the project’s objectives. The proposal suffers from an underdeveloped conceptual model, hypotheses that link only generally with the goals and objectives, methods that are not detailed enough to allow evaluation of their suitability for addressing the hypotheses, and some methods that seem poorly chosen, with justification lacking. The worthy goals of the project are in its favor, as the information gained if the methods were better developed and spelled out in detail could be very useful in directing management attention to areas supporting a high diversity and growing populations of grassland birds.**
Rating	**fair**